weights of each sort, according as n is even or odd; and that then any given weight can be expressed in a single way only if n is odd.
R. E. B.

I AM glad to have elicited a simple and direct rule for the required distribution, to obviate the necessity of a tedious tentative process, or of the reference to tables suggested in Mr. Willis's first letter. My own rule effected the desired object, but the device of admitting negative as well as positive remainders in the successive divisions by 3 is a decided improvement. R. E. B.'s method is identical with Mr. Willis's second method, and is undoubtedly the best. Its relation to my method is seen by noting that $\frac{1}{2}(2^7 - 1)$ is ternary 1111111, which, being added to $1\bar{1}01\bar{1}\bar{1}\bar{1}$, converts it into 2012000. My rule might have been generalized by adding (instead of *the least* value) *any* value of $\frac{1}{2}(2^n - 1)$ that exceeds the given weight.

In connection with Mr. Willis's suggestion (in his first letter) of tables for finding what number a person has thought of, I may mention that I published through Simpkin and Marshall, nearly forty years ago, a set of 4 cards for this purpose under the name of "Sibylline Leaves," of which a few specimens are still in my possession. Taking advantage of the fact that weights of 1, 3, 9, 27 will make up any integer from − 40 to + 40, that is 81 different integers when 0 is included, the numbers on the cards ran from 1 to 81. The computation consisted in taking 41 to start with, and adding or subtracting 1, 3, 9, or 27, two kinds of type being employed to distinguish between addition and subtraction.
J. D. EVERETT.

The Composition of Sea-Water.

COULD any reader of NATURE who may have given attention to the subject offer some explanation of the fact that the water of the sea contains such a very large excess of sodium salts relatively to salts of potassium?

Many of us have thought about the question, and heard it discussed, but I am not aware that any satisfactory conclusion has been arrived at.

I think it is usually assumed that the salts in the ocean have been mainly derived from the solutions carried in by rivers; solutions formed during the waste of rocks at the surface, or brought up by springs. Alkaline salts in such solutions will be principally due to the decay of felspars; and if we consider the rocks of the earth's crust, we find that the potash-felspars very much exceed in quantity the soda-felspars. It used to be considered that in earlier geological periods the "acid," mainly potash-bearing rocks, so enormously exceeded the "basic" rocks, in which the soda-felspars occur principally, that these latter were relatively quite insignificant in amount. Later petrographical work tends to show that this preponderance may not have been so large as was once supposed, but still there is no question that the excess of the potash-bearing rocks was, and is, very great. We might therefore look for more potash than soda in the drainage waters. We find, however, that in the sea, and in the rivers, the reverse is the case. Instances are, indeed, quoted (Roth, "Chemische Geologie") of rivers with more potash than soda in solution, but only as exceptions, and at points where only granite and gneiss had been drained.

This excess of soda in river-waters may be explained by the fact that though more potash-rocks are exposed than soda-rocks, yet the more rapid decay of the soda-lime felspars causes the proportions of the dissolved salts to be such as we find them; though this would hardly suffice to explain the great difference we find in the ocean.

Some people, again, assume that the composition of sea-water, though it may have been modified by the river-waters, is due to the constituents which were contained in the original first ocean as it condensed on the surface of the cooling earth—the "Urmeer" of the Germans; and Roth points out, in discussing analyses of river-waters and sea-water, that the composition of the former is such that they never could be the cause of the present composition of the latter.

This going back to the original ocean may be, as indeed it seems to be, the only explanation open to us, but it is not without its weak points. If potash-salts so greatly exceed soda-salts in the earth's crust now, we may assume that the same relative proportions existed, more or less, when the whole mass was still gaseous; and it is not easy to see any reason why. when cooling and condensation had allowed of the formation of a mass of molten silicates, the sodium-salts should have remained in great excess in the still uncondensed heated atmosphere out of which the "Urmeer" would eventually be deposited on the cooling crust. Mere difference of volatility of the respective salts would not suffice to account for this; and if we are to take the hypothesis at all, we must perhaps assume that when a low enough temperature had been reached to allow of the combination of the respective elements to molten silicates, the potassium, as the stronger base, would be taken more largely into these combinations by preference to the sodium, which would partly remain in the still intensely heated atmosphere, to be condensed with other vapours at a later period.
M.

Birds' Nests.

IN addition to your "curious places for birds' nests," I give you my own experience between 1842 and 1882 at Highfield House, Nottinghamshire.

Redbreast for four consecutive years on a shelf.
Redbreast in a fern (*Platycerium alcicome*) for four consecutive years.
Redbreast in a *Strelitzia regina* plant.
Hedge Warbler in a tall Fuchsia in a greenhouse.
Chiff Chaff in a fern (two years).
Pied Wagtail on a shelf in vinery (two years).
Flycatcher on hinge of door (ten consecutive years).
Flycatcher on ledge of thermometer stand (three years).
Wren in a Daniels's hygrometer stand.

County Club, Chepstow.
E. J. LOWE.

Butterflies Bathing.

IN answer to the inquiry of Mr. G. A. Freeman (NATURE, vol. xlii. p. 545) as to the food and habits of *Papilio macleayanus*, the butterfly which has been observed to visit water apparently for the purpose of performing its ablutions, I may inform him that the species is commonly found about Sydney, where it feeds in its larval condition on the camphor laurel (*Laurus camphora*), and the tender shoots and leaves of the orange. It certainly is not aquatic during any part of its life, nor do the plants upon which it feeds grow near water; the insect simply follows the example of its brothers, depositing its eggs singly, and undergoing the transformations on the food-plant as any reasonable butterfly should. Mr. G. Lyell's note as to the bathing habits of *P. macleayanus* is most interesting, and as far as I am aware the observation is entirely new, although many butterflies of the family Lycænidæ frequent pools on very hot days, settling on the mud at their margins, probably in search of a little moisture. Only recently at Toowoomba, in Queensland, I noticed a number of *Holochila absimilis* settled about puddles formed on the roads by a passing shower.

A. SIDNEY OLLIFF.

Department of Agriculture, Macquarie Street, Sydney,
November 11, 1890.

THE RESEARCHES OF DR. R. KŒNIG ON THE PHYSICAL BASIS OF MUSICAL SOUNDS.[1]

I.

NOT often does it fall to the lot of a scientific man to become the mouthpiece of another whose researches have lasted over a quarter of a century; yet this is the enviable position in which I find myself on this occasion as the spokesman of Dr. Rudolph Kœnig, who is known not only as the constructor of the finest acoustical instruments in the world, but as an investigator of great originality and distinction, and author of numerous memoirs on acoustics. Dr. Kœnig, who has of late made very important contributions to our knowledge of the physical basis of music, using apparatus immeasurably superior to any hitherto employed in experimental investigations of this subject, has on various occasions, when I have visited him in Paris, shown me these instruments, and repeated to me the results of his researches. Important as these are, they are all too little known in this country, even by the professors of physics. It was, therefore, with no little satisfaction that the Council of the Physical Society learned that Dr. Kœnig was willing to send over to London for exhibition on this occasion the instruments and apparatus used in these researches. And their satisfaction to-day is heightened by the fact that Dr. Kœnig has himself very kindly come over to demonstrate his own researches, and has given us the opportunity to welcome him personally amongst us.

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[1] By Prof. Silvanus P. Thompson. (Communicated by the author, having been read to the Physical Society of London, May 16, 1890.)

The splendid apparatus around me belongs to Dr. Kœnig, and forms but a very small part of the collection which adorns his *atélier* on the Quai d'Anjou. He lives and works in seclusion, surrounded by his instruments, even as our own Faraday lived and worked amongst his electric and magnetic apparatus. His great tonometer, now nearly completed, comprises a set of standard tuning-forks, adjusted each one by his own hands, ranging from 20 vibrations per second up to nearly 40,000, with perfect continuity, many of the forks being furnished with sliding adjustments, so as to give by actual marks upon them any desired number of vibrations within their own limits. Beside this colossal masterpiece, Dr. Kœnig's collection includes several large wave-sirens, and innumerable pieces of apparatus in which his ingenious manometric flames are adapted to acoustical investigation. There also stands his tonometric clock, a timepiece governed, not by a pendulum, but by a standard tuning-fork, the rate of vibration of which it accurately records.

It is not surprising that one who lives amongst the instruments of his own creation, and who is familiar with their every detail, should discover amongst their properties things which others whose acquaintance with them is less intimate have either overlooked or only imperfectly discerned. If he has in his researches advanced propositions which contradict, or seem to contradict, the accepted doctrines of the professors of natural philosophy, it is not that he deems himself one whit more able than they to offer mathematical or philosophical explanations of them: it is because, with his unique opportunities of ascertaining the facts by daily observation and usage, he is impelled to state what those facts are, and to propound generalized statements of them, even though those facts and generalized statements differ from those at present commonly received and supposed to be true.

At the very foundations of the physical theory of music stand three questions of vital importance:—

(1) Why is it that the ear is pleased by a succession of sounds belonging to a certain particular set called a scale?

(2) Why is it that when two (or more) musical sounds are simultaneously sounded, the ear finds some combinations agreeable and others disagreeable?

(3) Why is it that a note sounded on a musical instrument of one sort is different from, and is distinguishable from, the same note sounded with equal loudness upon an instrument of another sort?

These three queries involve the origin of *melody*, the cause of *harmony*, and the reason of *timbre*.

The theories which have been framed to account for each of these three features of music are based on a double foundation—partly physical, partly physiological. With the physiological aspect of this foundation we have to-night nothing to do, being concerned only with the physical aspect. What, then, are the physical foundations of melody, of harmony, and of timbre? Demonstrable by experiment they must be, in common with all other physical facts, otherwise they cannot be accepted as proven. What are the facts, and how can they be demonstrated?

We are not here, however, to fight over again the battle of the temperaments, nor do I purpose to enter upon a discussion of the origin of melody, which, indeed, I believe to be associative rather than physical. I shall confine myself to two matters only, with which the recent researches of Dr. Kœnig are concerned—the *cause of harmony* and the *nature of timbres*. Returning, then, to the ratios of the vibration numbers of the major scale, we may note that two of these, namely, the ratios 9 : 8 and 15 : 8, which correspond to the intervals called the major whole tone and the seventh, are dissonant—or, at least, are usually so regarded. It will also be noticed that these particular fractions are more complex than those that represent the consonant intervals. This naturally raises the question: *Why is it that the consonant intervals should be represented by ratios made up of the numbers* 1 : 6, *and by no others?*

To this problem the only answer for long was the entirely evasive and metaphysical one that the mind instinctively delights in order and number. The true answer, or rather the first approximation to a true answer, was only given about forty years ago, when von Helmholtz, as the result of his ever-memorable researches on the sensations of tone, returned the reply: *Because only by fulfilling numerical relations which are at once exact and simple can the "beats" be avoided which are the cause of dissonance.* The phenomenon of beats is so well known that I may assume the term to be familiar. An excellent mode of making beats audible to a large audience is to place upon a wind-chest two organ-pipes tuned to $ut_2 = 128$, and then flatten one of them slightly by holding a finger in front of its mouth. Von Helmholtz's theory of dissonance may be briefly summarized by saying that any two notes are discordant if their vibration numbers are such that they produce beats, maximum discordance occurring when the beats occur at about 33 per second; beats if either fewer than these, or more numerous, being less disagreeable than beats at this frequency. It is an immediate consequence that the degree of dissonance of any given interval will depend on its position on the scale. For example, the interval of the major whole tone, represented by the ratio 9 : 8, produces four beats per second at the bottom of the pianoforte keyboard, 32 beats per second at the middle of the keyboard, and 256 beats per second at the top. Such an interval ought to be discordant, therefore, in the middle octaves of the scale only.

To this view of von Helmholtz it was at first objected that, if that were all, all intervals should be equally harmonious provided one got far enough away from being in a bad unison: fifths, augmented fifths, and sixths minor and major, ought all to be equally harmonious. This no musician will allow. To account for this von Helmholtz makes the further supposition that the beats occur, not simply between the fundamental or prime tones, but also between the upper partials which usually accompany prime tones. This leads me to say a word about *upper partial tones* and *harmonics*. I believe many musicians use these two terms as synonymous; but they ought to be carefully distinguished. The term harmonics ought to be rigidly reserved to denote higher tones which stand in definite harmonic relations to the fundamental tone. The great mathematician Fourier first showed that any truly periodic function, however complex, could be analyzed out and expressed as the sum of a certain series of periodic functions having frequencies related to that of the fundamental or first member of the series as the simple numbers 2, 3, 4, 5, &c. Thirty years later, G. S. Ohm suggested that the human ear actually performs such an analysis, by virtue of its mechanical structures, upon every complex sound of a periodic character, resolving it into a fundamental tone, the octave of that tone, the twelfth, the double octave, &c. Von Helmholtz, arming himself with a series of tuned resonators, sought to pick up and recognize as members of a Fourier series, the higher harmonics of the tones of various instruments. In his researches he goes over the ground previously traversed by Rameau, Smith, and Young, who had all observed the co-existence, in the tones of musical instruments, of higher partial tones. These higher tones correspond to higher modes of vibration in which the vibratile organ—string, reed, or air-column—subdivides into two, three, four, or more parts. Such parts naturally possess greater frequency of vibration, and their higher tones, when they co-exist along with the lower or fundamental tone are denominated *upper partial tones*, thereby signifying that they are higher in the scale, and that they correspond to vibrations *in parts*. It is to be regretted that Prof. Tyndall, in his lectures on sound, rendered von Helmholtz's *Oberpartialtöne* by the term *overtones*, omitting the most significant half of the word. To avoid all confusion in the use of such a term I shall rather follow Dr. Kœnig in speaking of these as *sounds of subdivision*. And I must protest emphatically against calling these sounds harmonics, for the simple reason that in many cases they are very inharmonious. It is a matter to which I shall recur presently.

Returning to the subject of beats, the question arises, What becomes of the beats when they occur so rapidly that they cease to produce a discontinuous sensation upon the ear? The view which I have to put before you in the name of Dr. Kœnig is that they blend to make a tone of their own. Earlier acousticians have propounded, in accordance with this view, that the *grave harmonic* of Tartini (a sound which corresponds to a frequency of vibration, that is the difference between those of the two tones producing it) is due to this cause. Von Helmholtz has taken a different view, denying that the beats can blend to form a sound, giving reasons presently to be examined. Von Helmholtz considered that he had discovered a new species of combinational tone—namely, one corresponding in frequency to the *sum* of the frequencies of the two tones, whereas that discovered by Tartini (and before him by Sorge) corresponded to their *difference*. Accordingly, he includes under the term of combinational tones the differential tone of Tartini and the summational tone which he considered himself to have discovered. To the existence of such combinational tones he ascribed a very important part in determining the character, harmonious or otherwise, of chords; and to them also he attributes the ability of the ear to discriminate between the degrees of harmoniousness possessed by such intervals (fifths, sixths, &c.) as consist of two tones too widely apart on the scale to give beats of a discontinuous character. He also considers that such combinational tones are chiefly effective in producing beats, the summational tones of the primaries beating with their upper partial tones; and that this is the way in which they make an interval more or less harmonious.

The whole fabric of the theory of harmony, as laid down by von Helmholtz, is thus seen to repose upon the presence or absence of beats; and the beats themselves are in turn made to depend, not upon the mere interval between two notes, but upon the timbres also of those notes, as to what upper partials they contain, and whether those partials can beat with the summational tone of the primaries. It becomes, then, of the utmost importance to ascertain the precise facts about the beats and about the supposed combinational tones. What the numbers of beats are in any given case: whether they do or do not correspond to the alleged differential and summational tones: these are vital to the theory of harmony. Equally vital is it to know what the timbres of sounds are, and whether they can be accurately or adequately represented by the sum of a set of pure harmonics corresponding to the terms of a Fourier series.

In investigating beats and combinational tones, Dr. Kœnig deemed it of the highest importance to work with instruments producing the purest tones; not with harmonium reeds or with polyphonic sirens, the tones of which are avowedly complex in timbre, but with massive steel tuning-forks, the pendular movements of which are of the simplest possible character. Massive tuning-forks properly excited by bowing with a violoncello bow, or, in the case of those of high pitch, by striking them with an ivory mallet, emit tones remarkably free from all sounds of subdivision, and of so truly pendular a character (unless over-excited) that none of the harmonics corresponding to the members of a Fourier series can be detected. No living soul has had a tithe of the experience of Dr. Kœnig in the handling of tuning-forks. Tens of thousands of them have passed through his hands. He is accustomed to tune them himself, making use of the phenomenon of beats to test their accuracy. He has traced out the phenomena of beats through every possible degree of pitch, even beyond the ordinary limits of audibility, with a thoroughness utterly impossible to surpass or to equal. Hence, when he states the results of his experience, it is idle to contest the facts gathered on such a unique basis. The results of Dr. Kœnig's observations on beats are easily stated. He has observed primary beats, as well as beats of secondary and higher orders, from the interference of two simple tones simultaneously sounded.

When two simple tones interfere, the primary beats always belong to one or other of two sets, called an *inferior* and a *superior* set, corresponding respectively in number to the two remainders, positive and negative, to be found by dividing the frequency of the higher tone by that of the lower.

This mode of stating the facts is a little strange to those trained in English modes of expressing arithmetical calculations; but an example or two will make it plain. Let there be as the two primary sounds two low tones having the respective frequencies of 40 vibrations and 74 vibrations. What are the two remainders, positive and negative, which result from dividing the higher number, 74, by the lower number 40? Our English way of stating it is to say that 40 goes into 74 once, and leaves a (positive) remainder of 34 over. But it is equally correct to say that 40 goes into 74 twice all but 6: or that there is a negative remainder of 6. Well, Dr. Kœnig finds that, when these two tuning-forks are tried, the ear can distinguish two sets of beats, one rapid, at 34 per second, and one slow, at 6 per second.

Again, if the forks chosen are of frequencies 100 and 512, we may calculate thus: 100 goes into 512 five times, plus 12; or 100 goes into 512 six times, minus 88. In this actual case the 12 beats belonging to the inferior set would be well heard: the 88 beats belonging to the superior set would probably be almost indistinguishable. As a rule, the inferior beat is heard best when its number is *less* than half the frequency of the lower primary, whilst, when its number is *greater*, the superior beat is then better heard. Dr. Kœnig has never been able to hear any primary beat which did not fall within this rule.

Dr. Kœnig will now illustrate to you the beats, inferior and superior, as produced by these two massive tuning-forks,[1] each weighing about 50 pounds, and each provided with a large resonating cavity consisting of a metal cylinder, about 4 feet long, fitted with an adjustable piston. One of them is tuned to the note $ut_1 = 64$. The other also sounds ut_1; but by sliding down its prongs the adjustable weights of gun-metal, and screwing in the piston of the resonator, its pitch can be raised a whole tone to $re_1 = 72$. Dr. Kœnig excites them with the 'cello bow, first separately that you may hear their individual tones, then together. At once you hear an intolerable beating—the beats coming 8 per second. This is the inferior beat, corresponding to the positive remainder; the superior beat you cannot hear. Dr. Kœnig will raise the note of the second fork from re_1 to $mi_1 = 80$; and the beats quicken to 16 per second. Raising it to $fa_1 = 85\frac{1}{3}$, and then to $sol_1 = 96$, while the first fork is still kept at ut_1, the beats increase in rapidity, but are fainter in distinctness. If Dr. Kœnig now substitutes for the second fork one tuned to $la_1 = 106\frac{2}{3}$, you may be able to hear two beats, the inferior one rapid and faint at $42\frac{2}{3}$ per second, and the superior one slower, but also faint, at $21\frac{1}{3}$ per second. Still raising the pitch to the true seventh tone = 112, the rapid inferior beat has died out, but now you hear the superior strongly at 16 per second. If it is raised once more to $si_1 = 120$ (the seventh of the ordinary scale), and the beats are still stronger and slower at 8 per second. Finally, when we bring the pitch up to the octave $ut_2 = 128$, we find that all beats have disappeared: there is a perfectly smooth consonance. The facts so observed are tabulated for you as follows:—

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[1] These splendid forks, with their resonators, along with other important pieces of Dr. Kœnig's apparatus, have since been acquired by the Science and Art Department for the Science Collection at South Kensington.

TABLE I.

Primary Beats.

Primary Tones.	Ratio.	Inferior Beats.	Superior Beats.
ut_1 re_1 64 72	8 : 9	8	—
ut_1 mi_1 64 80	4 : 5	16	—
ut_1 fa_1 64 $85\frac{1}{3}$	3 : 4	$21\frac{1}{3}$	—
ut_1 sol_1 64 96	2 : 3	32	32
ut_1 la_1 64 $106\frac{2}{3}$	3 : 5	$42\frac{2}{3}$	$21\frac{1}{3}$
ut_1 (7) 64 112	4 : 7	—	16
ut_1 si_1 64 120	8 : 15	—	8
ut_1 ut_2 64 128	1 : 2	—	0

Suppose now, keeping the lower fork unaltered, we raise the pitch of the higher note (taking a new fork that starts at the octave) from ut_2 to sol_2 by gradual steps, we shall find that there begins a new set of primary beats—an inferior set, which are at first slow, then get more rapid and become undistinguishable, but succeeded by another rapid and indistinct, which grow stronger and slower, until, as the pitch rises to sol_2, the frequency of which is exactly three times that of ut_1, all beats again vanish. This range between the octave and the twelfth tone may be called the second "period," to distinguish it from the period from unison to the first octave, which was our first period. Similarly, the range from the twelfth tone to the second octave is the third period, and from thence to the major third above is the fourth period, and so forth. In each period, up to the sixth or seventh of such periods, a set of inferior and a set of superior beats may be observed; and in every case the frequency of the beats corresponds, as I have said, to one or other of the two remainders of the frequencies of the two tones. No beat has ever been observed corresponding to the sum of the frequencies, even when using the slowest forks. None has ever been observed corresponding to the difference of the frequencies, save in the first period; where, of course, the positive remainder is simply the difference of the two numbers.

That you may hear for yourselves the beats belonging to one of the higher periods, Dr. Kœnig will take a pair of forks which will give us some of the superior beats in the fourth period. One of the forks is the great $ut_1 = 64$, as previously used. The other is $mi_3 = 320$; their ratio being 1 : 5. Sounded together they give a pure consonance, but if the smaller one is loaded with small pellets of wax to lower its pitch slightly, and then bow it, at once you hear beats. It was in studying the beats of these higher periods that Dr. Kœnig made the observation that whereas the beats of an imperfect unison are heard as alternate silences and sounds, the beats of the (imperfect) higher periods—twelfth tone, double octave, &c.—consist mainly in variations in the loudness of the lower of the two primary tones; an observation which was independently made by Mr. Bosanquet, of Oxford.

Passing from the beats themselves, I approach the question, What becomes of the beats when they occur too rapidly to produce on the ear a discontinuous sensation? On this matter there have been several conflicting opinions: some holding, with Lagrange and Young, that they blend into a separate tone; others, with von Helmholtz, maintaining that the combinational tones cannot be so explained, and arise from a different cause. Let it be observed that, even if beat-tones exist, it is quite possible for beats and beat-tones to be simultaneously heard. A similar co-existence of a continuous and a discontinuous sensation is afforded by the familiar experiment of producing a tone by pressing a card against the periphery of a rapidly-rotating toothed wheel. There is a certain speed at which the individual impulses begin to blend into a continuous low tone, while yet there are distinguishable the discontinuous impulses; the degree of distinctness of the two co-existing sounds being dependent on the manner in which the card is pressed against the wheel—that is to say, on the nature of the individual impulses themselves. The opponents of the view that beats blend into a tone state plainly enough that, in their opinion, a mere succession of alternate sounds and silences cannot blend into a tone different from that of the beating tone. Having said that the beats cannot blend, they then add that they do not blend; for, say they, the combinational tones are a purely subjective phenomenon. Lastly, they say that even if the beats blend they will not so explain the existence of combinational tones, because the combinational tones have frequencies which do not correspond to the number of the beats.

In the teeth of all these views and opinions, Dr. Kœnig—without dogmatizing as to how or why it is—emphatically affirms that beats do produce *beat-tones*; and he has pursued the matter down to a point that leaves no room for doubting the general truth of the fact. The alleged discrepancy between the frequency of the observed combinational tones and that of the beats disappears when closely scrutinized. Those who count the beats by merely taking the difference between the frequencies of the two primary tones, instead of calculating the two remainders, will assuredly find that their numbers do not agree in pitch with the actual sounds heard. But that is the fault of their miscalculation. Those who use harmonium reeds or polyphonic sirens instead of tuning-forks to produce their primary tones must not expect from such impure sources to reproduce the effects to be obtained from pure tones. And those who say that the beats calculated truly from the two remainders will not account for the summational tones have unfortunately something to unlearn—namely, that, when pure tones are used, under no circumstances is a tone ever heard the frequency of which is the sum of the frequencies of the two primary tones.

The apparatus which Dr. Kœnig has brought over enables him to demonstrate, in a manner audible, I trust, to the whole assembly in this theatre, the existence of the beat-tones. His first illustrations relate to tones of primary beats, some belonging to the inferior, others to the superior set, in the first period.

He takes here the fork $ut_6 = 2048$, five octaves higher than the great ut_1. To excite it, he may either bow it or strike it with an ivory mallet. With it he will take the fork one note higher, $re_6 = 2304$. When he took the same interval with ut_1 and re_1, the number of beats was 8.

The *ut* and *re* of the next octave higher would have given us 16 beats, that of the next 32, that of the next 64, of the fourth octave 128, and that of the fifth 256. But 256 per second is a rapidity far too great for the ear to hear as separate sounds. If there were 256 separate impulses, they would blend to give us the note $ut_3 = 256$. They are not *impulses*, but *beats:* nevertheless, they blend. Dr. Kœnig strikes the ut_6, then the re_6, both shrill sounds when you hear them separately; but when he strikes them in quick succession one after the other, at the moment when the mallet strikes the second fork you hear this clear ut_3 sounding out. I am not going to waste your time in a disputation as to whether the sound you hear is objective or subjective. It is enough that you hear it, pure and unmistakable in pitch. It is the grave harmonic; and the number 256, which is its frequency, corresponds to the positive remainder when you divide 2304 by 2048.

Now let me give you a beat-tone belonging to the superior set: it also will be a grave harmonic, if you so please to call it; but its frequency will correspond neither to the difference nor to the sum of the frequencies of the two primary tones. Dr. Kœnig takes $ut_6 = 2048$ as previously, and with it $si_6 = 3840$. Let us calculate what the superior beats ought to be. 2048 goes into 3840 twice, less 256. Then, 256 being the negative remainder, we ought to hear from these two forks the beat-tone of 256 vibrations, which is ut_3, the same note as in our last experiment. He strikes the forks, and you hear the result. The beat-tone, which is neither a differential tone nor a summational tone, corresponds to the calculated number of beats.

If I take $ut_6 = 2048$ and $sol_6 = 3072$, the two remainders both come out at 1024, which is ut_5. Dr. Kœnig will first sound ut_5 itself, separately, on an ut_5 fork, that you may know what sound to listen for. Its sound has died away; and now he strikes ut_6 and sol_6, when at once you hear ut_5 ringing out. That sound which you all heard corresponds to the calculated number of beats. That is enough for my present purpose.

The next illustration is a little more complex. I select a case in which the beat-tones corresponding to the inferior and the superior beats will both be present. We shall have four tones altogether—two primary tones and two beat-tones. The forks I select are $ut_6 = 2048$, as before, and a fork which is tuned to vibrate exactly 11 times as rapidly as ut_3—it is the 11th harmonic of that note, but does not correspond precisely to any note of the diatonic scale. It has 2816 vibrations, and is related to ut_6 as 11 : 8. The two remainders will now be 768 and 1280, which are the respective frequencies of sol_4 and mi_5. Dr. Kœnig will first sound those notes on two other forks, that you may know beforehand what to listen for. Now, on striking the two shrill forks in rapid succession, the *two* beat-tones are heard.

If I select, instead of the 11th harmonic, the 13th harmonic of ut_3, vibrating 3328 times in the second, to be sounded along with ut_6, the same two beat-tones will be produced as in the preceding case; but $mi_5 = 1280$ is now the inferior one, corresponding to the positive remainder, whilst $sol_4 = 768$ is the superior tone, corresponding to the negative remainder. It is certainly a striking corroboration of Dr. Kœnig's view that the beat-tones actually heard in these last two experiments should come out precisely alike, though on the old view, that the combinational tones were simply the summational and differential tones, one would have been led to expect the sounds in the two experiments to be quite different.

One other example I will give you of a beat-tone belonging to the second period. The two primary notes are given by the forks $ut_5 = 1024$ and $re_6 = 2304$. The beat-tone which you hear is $ut_3 = 256$, which corresponds to the positive remainder.

It will be convenient to draw up in tabular form the results just obtained. These may be considered as abbreviations of the much more extended tables drawn up by Dr. Kœnig, which hang upon the walls, and which are to be found in his book, "Quelques Expériences d'Acoustique."

TABLE II.

Sounds of Primary Beats.

Primary Tones.			Ratio.	Inferior Beat-tone.	Superior Beat-tone.
ut_6 2048	re_6 2304	...	8 : 9	$1\left\{\begin{matrix} ut_3 \\ 256 \end{matrix}\right.$	—
ut_6 2048	si_6 3840	...	8 : 15	—	$1\left\{\begin{matrix} ut_3 \\ 256 \end{matrix}\right.$
ut_6 2048	sol_6 3072	...	8 : 12	$4\left\{\begin{matrix} ut_5 \\ 1024 \end{matrix}\right.$	$4\left\{\begin{matrix} ut_5 \\ 1024 \end{matrix}\right.$
ut_6 2048	(11th) 2816	...	8 : 11	$3\left\{\begin{matrix} sol_4 \\ 768 \end{matrix}\right.$	$5\left\{\begin{matrix} mi_5 \\ 1280 \end{matrix}\right.$
ut_6 2048	(13th) 3328	...	8 : 13	$5\left\{\begin{matrix} mi_5 \\ 1280 \end{matrix}\right.$	$3\left\{\begin{matrix} sol_4 \\ 768 \end{matrix}\right.$
ut_5 1024	re_6 2304	...	4 : 9	$1\left\{\begin{matrix} ut_3 \\ 256 \end{matrix}\right.$	—

(*To be continued.*)

THE ORIGIN OF THE GREAT LAKES OF NORTH AMERICA.

At one time glaciers—perhaps in the co-operative society of an ice-sheet—were gravely suspected of having excavated even the great lakes of North America. This, however, is hardly probable. The *a priori* difficulties in the hypothesis are great. Apart from objections which have often been pointed out, the work done would be on so gigantic a scale that a longer period must be assigned to the glacial occupation of the region than seems probable from other considerations. Further, the direct evidence which will presently be noticed seems conclusive against the hypothesis; but it may be affirmed with better reason that ice has indirectly aided in the process, though to what extent we can, as yet, hardly venture to say.

During the last few years numerous observations have been made, both in Canada and in the United States, upon the configuration of the lake beds, and the elevation of their ancient margins. To some of these Dr. Wright refers in his volume on "The Ice Age in North America," and Prof. J. W. Spencer (who has been engaged on this subject for several years) brings them into a focus in a paper recently published in the Quarterly Journal of the Geological Society of London.[1]

At first sight the great lakes, from Superior to Ontario, are suggestive of glacial excavation. They seem to occupy true rock basins. Superior discharges into Huron over the ledges—once a "portage"—of Sault Ste. Marie. Huron, as it were, leaks into Erie, the fall between the two sheets of water being only nine feet. Erie flows towards Ontario over the rocky rapids and the final precipice of Niagara; and the St. Lawrence, after leaving Ontario, gives frequent evidence of a rocky bed, the level of which is considerably above that of the bottom of the lake, for the depth of this near its eastern end is more than seven hundred feet. But more careful investigation of the lakes has shown that in these apparently perfect basins (as is sometimes discovered in household affairs) hidden cracks exist, which, under different physical conditions, would have let the water run out. This indeed is not the whole story; another agency must be presently mentioned; but that these apparent basins once had

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[1] Vol. xlvi. p. 523 (read April 16, 1890).